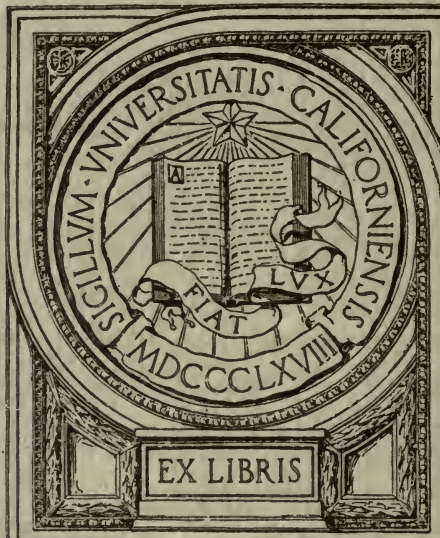

BALLISTIC EXPERIMENTS

FROM

1864 TO 1880

BY

FRANCIS BASHFORTH, B.D.,

HONORARY FELLOW OF ST JOHN'S COLLEGE, CAMBRIDGE;
LATE PROFESSOR OF APPLIED MATHEMATICS TO THE ADVANCED CLASS OF R.A. OFFICERS, WOOLWICH;
AND FORMERLY FELLOW OF ST JOHN'S COLLEGE, CAMBRIDGE

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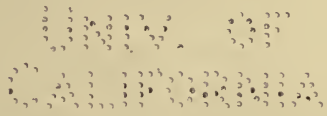

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INSTITUTION OF THE ADVANCED CLASS FOR ROYAL ARTILLERY OFFICERS.

When the Council of Military Education were preparing to establish the Advanced Class for Royal Artillery Officers, in 1863, they kindly invited me to become a candidate for the office of Professor of Applied Mathematics. But, as I was otherwise engaged, I felt obliged to decline the invitation. However, when they pressed me a second time, I had considered the matter, and found that it would be possible to determine the resistance of the air to the motion of projectiles by the use of a chronograph specially devised for that purpose. With proper assistance, it seemed to me that this work might be accomplished in about two years. In the Government application for temporary leave of non-residence for me it was stated that:

"No other candidates could be found (although the Council "pushed their enquiries in every direction) possessing to the "same extent the requisite combination of attainments. Should "Mr Bashforth's services, in consequence of the obstacle before "mentioned, be not available, the Council are of opinion that the "loss to the public service will be very great:..."

When I arrived at Woolwich, April 1864, I found the President and Vice-President of the Ordnance Select Committee were very decidedly opposed to any new chronograph. They had made numerous experiments, and could furnish me with any quantity of results from their stores! Chronographs with rotating cylinders had been tried and had failed! &c. It was clear that the O.S.C. knew nothing whatever about the application of mathematics to ballistics.

I soon came to the conclusion that if my proposed chronograph was to be used I must provide my own instrument. But still,

I felt that it would be only prudent to ascertain whether, if I provided my own chronograph, facilities would be afforded me for *trying* it, and for *experimenting* with it. A favourable reply was promptly received from the Director of Ordnance, who concluded by stating:

"I am to add that Earl de Grey and Ripon recognises with "much pleasure the zealous and practical manner in which you "have entered upon your duties." May 17th, 1864. (84-B-1100.)

The construction of the chronograph was commenced in my own workshop during our August Vacation, 1864, and it was reported ready for trial with ten equidistant screens in June 1865. But it was not till the following November when the Select Committee thought proper to afford me an opportunity to try my new invention in Plumstead Marshes.

I. First day, Nov. 23, rounds 1, 2, and 5 were very good, but 3 and 4 were defective. Second day, Nov. 29, round 7 was the only good one, 6 and 9 were fired by mistake, and 8 was defective. For the third and last day, Dec. 12, new screens had been provided, when rounds 10, 11, 13, 15, 16, 17, and 18 were good, and only 12 and 14 defective. The trial was considered quite satisfactory, and the results were reported to the Council of Military Education, Dec. 18, 1865, who had them printed for private circulation.

A full account of the chronograph was communicated to the Royal Artillery Institution, Woolwich, which was printed in their *Proceedings* for Aug. 1866, pp. 161–192. This account was also published separately.

In Feb. 1866 it was arranged that an experiment should be made with the new chronograph to determine the resistance of the air to five different forms of heads of elongated projectiles. The forms selected were: (1) Spherical; (2) Spheroidal; (3) Ogival, struck with a radius of *one* diameter; (4) Ogival, struck with a radius of *two* diameters; and (5) an approximation to the form of least resistance.

II. Rounds (1–13) were fired at Shoeburyness on Sep. 25, 1866; rounds (14–31) on Sep. 26; and rounds (32–43) on Sep. 27.

The firing of the gun was often interrupted by the passage of barges, and on Sep. 28 not a single round could be safely fired on account of these barges. At last the experiment was closed by firing the 44th round into the sea, so that only 43 rounds of shot were fired on three days out of the 70 rounds prepared for the experiment. It was never satisfactorily explained how it happened that these *barges* interfered on *every day* appointed for my experiment. Full particulars of each round were given in my Report, dated Oct. 23, 1866.

The remarks made on this Report by the Select Committee showed plainly that they did not know what they were about. They objected that the velocities in one group varied as much as 73·4 f.s., and thereupon expressed *their opinion*, that Navez's instrument would give the "*best absolute velocities*"! Now, if the Select Committee had taken the trouble to *read my Report* before criticising it, they would have found a simple solution of their difficulty. For, when round 32 was first mentioned, it was plainly stated *gun loaded the day before.* The muzzle velocity in the next round, the 33rd, was also sensibly reduced, apparently from the same cause. The Select Committee had to withdraw their objection. The Report of this my first experiment with the new chronograph concluded:

"As an indication of the practicability of working the new "chronograph with rapidity and certainty, I give the following "times of firing several successive rounds when there was *no* "*interruption.*

"Round	23,	Sep. 26, 1866	2^h	30′	30″	giving 9	records
,,	24	,,	2	37	20	,, 10	,,
,,	25	,,	2	44	3	,, 10	,,
,,	26	,,	2	49	35	,, 10	,,
,,	27	,,	2	55	0	,, 10	,,
,,	28	,,	3	0	20	,, 10	,,
,,	29	,,	3	5	10	,, 10	,,
,,	30	,,	3	11	3	,, 10	,,
,,	31	,,	3	15	20	,, 10	,,
Giving 9 rounds in			0	44	50	and 89	records."

A paper founded on the results of these experiments was communicated to the Royal Society, which was printed in the *Philosophical Transactions*, 1868, pp. 417–441.

Before proceeding further, it will be well to consider what progress the Ordnance Select Committee had made towards the determination of the resistance of the air to the motion of elongated projectiles. The Reports of their ballistic officers were as follows:

"The experiments referred to in this Report have regard "chiefly to initial velocity alone, and for the small distance con- "cerned the law of resistance adopted may be thought of small "practical importance, especially as before the experiments now "being carried on are concluded, the Committee will doubtless be "in a position to say whether this law is better expressed by a "function of the form $v^2 + \alpha v^3$, as proposed by General Piobert, or "by one of the form $v^2 + \beta v^4$, as proposed by the Count de St "Robert and Colonel Mayevski."

Captain Andrew Noble, *Report*, p. 3, 1862.

And again:

"It is regretted that this subject cannot be fully treated in "the present Report, but the difficulties in the way of a clear "solution are so many, and so great, that it would be difficult, "with our present experience, to assign any new law representing, "*with accuracy*, the resistance of the air to the motion of spherical "and elongated projectiles."

Lieut. W. H. Noble, R.A., *Report*, p. 19, April 2, 1865.

From this it is clear that, down to April 2, 1865, the Ordnance Select Committee and their ballistic officers had done nothing, by experiment, towards determining the resistance of the air to the motion of projectiles.

My determination of the resistance of the air for velocities 1,100 to 1,500 f.s., to four forms of the heads of elongated shot, was referred from the War Office to their Select Committee for their observations about Jan. 1867. And in the following May and June the Select Committee instituted experiments of the same kind, for one form of head. Their results were communicated to *The Engineer*, Nov. 15, 1867, claiming to have anticipated my results. Of course I complained to the Select Committee, but in vain. However, a higher authority very properly ordered them to insert in their *Proceedings* the following acknowledgement:

"NOTE—Referring to Minute 23,351, p. 255, Vol. V.

"The conclusions reported to the Ordnance Select Committee, "with reference to the resistance of the air to the passage of pro- "jectiles within the limits of velocities of 1,500 to 1,100 feet, and "recorded in § 2 of the above minute, are in accordance with the "law and co-efficient previously derived by the Rev. F. Bashforth, "B.D., Professor of Applied Mathematics to the Advanced Class "of Royal Artillery Officers, from experiments with his clock "chronograph, and communicated in reports of December 1865, "and October 1866."

Proceedings of the O.S.C., 1868, p. 397.

Immediately after, the Select Committee proceeded to attempt to set aside this decision of their superiors by their Minute 26,169, 21/10/68, referring to some experiments, said to have been made by them in Nov. and Dec. 1864. Their results were communicated to *The Engineer*, as before, April 30, 1869. But in the following week a disclaimer appeared in *The Engineer*. "The experiments "referred to in page 301, line 30, were not officially published "until 1868; the author therefore cannot, *strictly speaking*, claim "any priority"! I at once sought and obtained permission for my opponents to lay their claims, if they had any, before the Referees, who were considering our last experiments. In my printed Reports the date of every round is specified.

The Secretary of State for War ordered the dissolution of the Ordnance Select Committee on Dec. 3, 1868, when its President, Major-General Lefroy, R.A., was appointed Director-General of Ordnance.

The Secretary of State for War, in April 1867, sanctioned a course of experiments with the Bashforth Chronograph to determine the resistance of the air to the motion of elongated projectiles. It was arranged that these experiments should be carried out by

The Director of Artillery Studies,
The Professor of Artillery at the R.M.A.,
The Professor of Applied Mathematics,
And two Officers of the Advanced Class.

The two senior officers of the Advanced Class, 1868, Captain Morgan, R.A., and Captain Ford, R.A., were allowed to remain at Woolwich to assist in carrying out these experiments. But when Captain Morgan, R.A., obtained an appointment, his place was taken by Lieut. Sladen, R.A. These officers rendered most valuable assistance in all departments of the work, and became well qualified to carry out experiments with the new chronograph. The estimated expense of these experiments was £400.

The bores of the guns employed were 3, 5, 7, and 9-inch. The form of the head of the elongated projectiles used in these experiments was ogival struck with a radius of $1\frac{1}{2}$ × diameter of the shot. But the results thus obtained by experiment could be easily adapted to all other useful forms of heads of elongated shot.

III. The experiments commenced in Oct. 1867 were carried out at intervals in 13 days by firing about 200 rounds. Of these, about 30 rounds were defective*. The Report, dated July 23, 1868, concluded as follows:

"...and further, as the results obtained are of high scientific "importance, involving mathematical questions of considerable "difficulty, we would suggest that it may be referred to mathe- "maticians of eminence, such as the Astronomer Royal, Professor "Adams, Director of the Cambridge Observatory; *or* Professor "Stokes, Secretary of the Royal Society.

"Signed,

"Francis Bashforth, Professor of Applied Mathematics.
"C. F. Young, Captain R.A. and Major, Director of "Artillery Studies.
"Charles H. Owen, Captain R.A. and Lieut.-Colonel, "Professor of Artillery R.M.A.
"J. P. Morgan, Captain R.A.
"A. Ford, Captain R.A."

* When these experiments were made, the muzzle velocity had to be changed as soon as five or six good rounds had been obtained with one muzzle velocity. This called for a new charge and a new laying of the gun, and this increased the usual number of *defective* rounds.

Similar experiments were carried out afterwards with spherical shot, using the same bores of guns. About 200 rounds were fired at intervals in 13 days, but 27 rounds were defective. These experiments gave coefficients of resistance for velocities 850—2,100 f.s. The Report was dated Feb. 13, 1869.

At the conclusion of the above Report it was stated that, in order to obtain more satisfactory results for *elongated* projectiles, the times of passing each screen were being calculated to *five* places of decimals of a second. These experiments gave coefficients of resistance for velocities 900—1,700 f.s.

The above-mentioned Reports were printed and referred, not to *one*, as we intended, but to all the three distinguished mathematicians named by us in our Report, with the addition of the name of Captain Andrew Noble, a former ballistic officer of the O.S.C.

Appointment of Committee of Reference on the Subject of the foregoing Reports*.

84/B/1731.

War Office,
December 16, 1868.

Sir,

The Professor of Applied Mathematics to the advanced class of artillery officers, Woolwich, Rev. F. Bashforth, has reported to the Secretary of State for War the result of a series of experiments which he has conducted at the public expense, but with a chronograph of his own invention and construction, for the purpose of determining the law of the resistance of the air to the motion of a projectile. Mr Bashforth has further represented that the results obtained are of high scientific importance, involving mathematical questions of considerable difficulty, and has asked that they may be referred to mathematicians of eminence for an opinion on their precision and value. The Secretary of State having acceded with pleasure to this request, I am directed to

* Report No. IV., dated 13th Feb. 1869, was referred to the Committee subsequently to the letter of appointment.

ask the favour that you will act as one of the referees, the others applied to being the gentlemen named in the margin.

Mr Cardwell desires to be informed by an independent scientific committee of reference (1), whether it is now to be considered as proved that the resistance of the air varies practically as the cube of the velocity of the shot for all the velocities in use in gunnery, ranging from 300 to 1,900 feet per second, or for what range of velocities; and if not true, whether it is nearer the truth than any equally simple law before propounded?

(2) Whether this law of resistance is to be regarded as a new one, the discovery of which is due to Mr Bashforth?

(3) Whether the instrument devised and perfected by that gentleman for recording successive small intervals of time is susceptible of general employment at schools of instruction in gunnery?

(4) Whether any means of solving the same problem with equal precision existed before?

Upon the answers to these questions will depend in some degree the credit due to Professor Bashforth for his public and private exertions in solving this important problem, and it is his desire, as well as that of the Secretary of State, that his claims to the scientific distinction of having been the first philosopher to discover, or at least to prove, the true law of resistance so long sought for, shall rest on the unbiassed decision of competent mathematicians.

The Secretary of State would wish the referees to agree to the tenor of a general answer to each of the above questions, but will be happy to receive separate reports, if desired, or opinions on the general question from other points of view.

I am, &c.

J. H. LEFROY,
Major-General, Director-General of Ordnance.

Geo. Biddell Airy, Esq., Royal Observatory.
Professor Adams, Cambridge Observatory.
Professor Stokes, Royal Society.
Captain A. Noble.

ROYAL OBSERVATORY, GREENWICH, S.E.,
March 11, 1869.

MY DEAR SIR,

I ask leave to place before you my position in reference to the Bashforth Commission.

I have taken such opportunities as presented themselves for making myself acquainted with the principal ballistic-measurers. Professor Bashforth's, which I have seen in action (as well as by private examination), I know pretty well; the Navez-Leurs (which I have seen almost as well), I fairly understand; the other instruments less perfectly. But on the result of this I have no doubt whatever that the Bashforth instrument is far superior to the others.

I have endeavoured to look to the records of past experiments, but I have not leisure to look to them properly. I can see clearly that for practical military purposes there exists, or may at any moment be obtained, all that military men can desire. But for any clear philosophical result as to the law of resistance at low velocities, the law of resistance at and about sound-velocity, and the law of resistance at high velocities,—as a connected series of determinations,—much must be done, and a longer time and greater labour must be expended than I should like to pledge myself to.

My difficulties are much increased by the wide geographical separation from my colleagues.

Under these circumstances I should be glad if you would sanction my retiring from the Commission. I think that if it were understood that Professor Stokes is the leader, he would with advantage (particularly from his proximity of residence to Professor Adams) enter upon the philosophical points, which are nearly related to his favourite studies, and there would be good prospect of a result beneficial to science.

I am, my dear Sir,

Yours very truly,

G. B. AIRY.

General Lefroy, R.A.

Report of the Committee of Reference, 1870.

84/B/1902.

Cambridge,
April 18, 1870.

Sir,

We beg leave to state that we have carefully considered the questions proposed to us in your letter numbered 84/B/1731, respecting the merits of Professor Bashforth's chronograph and the value of the experiments conducted by means of it.

The principal chronographs prior to that of Professor Bashforth which have of late years been used for artillery purposes are,—

(1) That invented by Major Navez, and subsequently improved by Colonel Leurs, in which the time is measured by the arc passed over by a pendulum. With this may be classed certain other instruments, as that of Major Benton and Captain Vignotti, in which the pendulum principle is employed in a similar manner. In the last the registration is effected by the spark of an induction coil, instead of, as in the others, by a mechanism depending on the throwing of an electro-magnet out of or into action.

(2) That of Capt. Boulengé, in which the time is measured by the space passed over by a falling weight.

(3) That of Capt. Schultz, in which the time is measured by the arc registered by two induction sparks on a revolving cylinder, which arc is converted into time by means of the undulating mark registered on the cylinder by a fork vibrating so as to give a musical note of known pitch.

The above instruments, with the exception of that of Capt. Schultz, are adapted to measure only a single interval of time; that of Capt. Schultz alone is able to measure any number of consecutive intervals.

For certain objects of artillery experiments (as, for example, the testing of gunpowder), the determination of a single velocity, involving the measure of a single interval of time, is all that is required.

For others (as, for example, for determining the resistance of the air to projectiles, and the law of that resistance as varying

with the velocity), it is essential to measure two or more velocities in the course of the projectile's motion.

In expressing an opinion as to the relative merits of Professor Bashforth's and other chronographs, we think it desirable to keep these two classes of objects distinct.

We will first consider the determination of a single velocity.

In order that the results should be perfectly satisfactory they should satisfy two distinct conditions :—

(1) Under identical circumstances the results should be the same, or rather, since every observation is necessarily subject to slight error, should present only very small differences, a comparison of which affords means of determining the variability of the indication of the instrument.

(2) They should be unaffected by any error which, being constant under similar circumstances, is not determined by a comparison of a series of observations, nor eliminated by taking their mean.

The degree of constancy of the indications of an instrument under identical circumstances may be estimated,—

(1) By repeated observations of intervals of time which we know, *à priori*, to be equal to each other, such as the time taken by a given body to fall freely through a given space.

(2) By observing identical intervals of time by means of two or more instruments of the same kind.

(3) By the regularity of the measures of a series of consecutive intervals of time which from their nature change continuously.

In instruments which, like the Navez-Leurs, measure only one interval of time, we can only employ the first two of these methods, while in Schultz's and Bashforth's instruments, which are capable of measuring any number of consecutive intervals of time, we gain the important advantage of being able also to employ the third method, so that each separate experiment supplies means of testing the regularity of the indications of the instrument.

The experiments which have been laid before us, in which the time of flight of a given projectile over the same space is recorded by three of the Navez-Leurs instruments, show a remark-

able degree of regularity in the indications of those instruments, a degree of regularity much greater, in fact, than might have been expected.

The experiments recorded in Professor Bashforth's description of his chronograph, as well as those given in his paper published in the second part of the Philosophical Transactions for 1868, afford ample materials for judging of the regularity of the indications of his instrument, and show that this regularity is everything that can be desired.

We have not been able to obtain experiments for testing in a similar way the degree of constancy in the indications of the Schultz instrument.

The next point which we have to consider is the comparative freedom of the several instruments from liability to constant errors.

With respect to this point, we must observe that neither the Navez-Leurs nor the Boulengé instrument is of the same *strictly differential* character as the Schultz or Bashforth. Hence, however uniform either of the two former instruments may be in its working, we still require evidence, beyond what is furnished by the instrument itself, of the correctness of its indications regarded absolutely, not relatively, as far, at least, as conceivable sources of error exist which do not operate strictly in the same way in the two observations on which each result depends.

The simultaneous employment of several instruments of the same pattern affords means of detecting and eliminating such constant errors, if any, as depend on an individual instrument, but not such, if any, as belong to the construction.

The simultaneous employment of instruments of very different constructions affords a more searching mode of detecting constant errors, if such exist, except in so far as the constructions involve a common element which might be conceived to influence the result.

The evidence of the absence or minuteness of such errors will of course increase with the number and the dissimilarity of the constructions employed. The agreement of even two *dissimilar* instruments renders it probable that both are correct or nearly so, though of course it is *possible* that the two might err to nearly the same amount.

In the Navez-Leurs and the Boulengé instruments the means of recording are very different, except as to the employment of the electro-magnets, which is the same in both. A series of comparisons in which the same velocities (ranging from about 930 to 1,015 f.s.) were taken simultaneously with two instruments (one of each of those kinds) have been laid before us, which show a very near agreement, the difference averaging only about 3·5 f.s.

The differences are, however, mostly of one sign, indicating that one at least of the two is subject to an appreciable, though probably minute, constant error.

Again, intervals of time which may be deemed identical have been measured by the Bashforth chronograph and by each of two Navez-Leurs instruments (the interval measured by the Bashforth and one Navez-Leurs being however different from that measured by the Bashforth and the other Navez-Leurs), and though the differences are not considerable, they are in the case of one of the Navez-Leurs instruments almost all of the same sign, indicating a small constant error in the case of one of the instruments (the Bashforth and that Navez-Leurs) compared. And since the Navez-Leurs instrument is not of the same *strictly differential* character as the Bashforth chronograph, we can hardly hesitate to attribute *to it* the small constant error detected.

In the Navez-Leurs instrument the interval of time is measured by means of the observed arc described by a pendulum during that time. Now the time of describing any given arc is of course affected by friction, and in order to take this into account, the time of describing the same arc by a simple pendulum unaffected by friction is multiplied by a factor, the value of which is found by observing the instrumental measures which correspond to intervals of time which are known *à priori*, such as the time of a body falling freely through a given small space. It is found however that the value of this factor is very sensibly different for different parts of the arc of oscillation of the pendulum. Practically the factor is determined, by means of falling weights with which the instrument is furnished, for a considerable arc, beginning a certain distance below the starting point of the pendulum, then for another considerable arc beginning where

the former ended, and similarly for a third; and in the conversion of an arc actually observed in the use of the instrument into time, the different values thus obtained are applied to the corresponding parts of the total arc described by the pendulum. On account of the very sensible variation of the factor, it may be doubted whether this mode of converting arc into time possesses a degree of exactness answering to the delicacy of the instrumental indications. Thus, while indications nearly equal to each other may be compared by means of this instrument with great accuracy, we do not think that quite the same confidence can be placed in its determinations of absolute velocity, or of relative velocity when the difference between them is considerable. The case is somewhat similar to that of comparing different temperatures by means of a very sensitive thermometer, which notwithstanding very sensible variations of bore has been calibrated on the assumption that for very considerable portions of the interval which separates the standard points the bore may be taken as uniform.

In Schultz's instrument an interval of time is measured by the number of vibrations made by a vibrating fork, which, for example, in the particular instrument described by Colonel Benét, makes about 249 double vibrations per second. Since the time corresponding to a small fraction of a vibration is observable by means of this instrument, it is obvious that the indications of the instrument are exceedingly delicate, and that if we could depend on the vibrations of the fork being all performed in exactly equal times, no more accurate time measure could possibly be desired.

The vibrations of the fork are kept up by the action of two electro-magnets, which are excited by a galvanic current, which is alternately closed and interrupted during each vibration of the fork, by means of the motion of a lever whose oscillations are performed in the same time as those of the fork.

Now it is to be remarked that this action of the electro-magnets on the vibrating fork constitutes a disturbing force which interferes with the perfect uniformity of its vibrations. In experiments described by Colonel Benét the number of double vibrations per second was found to vary from 246 to 253. The number of vibrations also varies in consecutive seconds in such a

way as to show that the period of the disturbances is much less than a second, and that therefore during different parts of the same second the fork may be vibrating at sensibly different rates.

For this reason the time measures made by this instrument cannot be regarded as at all commensurate with the extreme delicacy of the indications of the instrument.

In Professor Bashforth's chronograph the time is directly measured by means of a clock with a half-seconds pendulum. Every double vibration of the pendulum is recorded on the revolving cylinder by the breaking of the same galvanic current under exactly the same conditions, and the time of the cannon balls passing through each of the screens is recorded on the same revolving cylinder by the *momentary* interruption of a second galvanic current under exactly the same conditions.

Thus the time scale required for converting the records made by this instrument into time intervals is here *supplied by the instrument itself*, and consequently the results obtained by it are unaffected by any error of the same kind as those to which we have seen the former instruments to be liable. This freedom from constant errors and from errors of reduction is one of the great advantages of Professor Bashforth's instrument.

Next we have to consider the determination of the velocities at several points of the path of the same projectile.

We have already stated that Schultz's instrument is the only one which competes in this respect with that of Professor Bashforth.

The objection that the rate of vibration of the fork in Schultz's instrument is to a certain extent variable, probably on account of the action of the electro-magnets, applies in this case with its full force, for since the period of disturbance in the rate of vibration is very short, it might very well happen that the velocity near the beginning of the flight of the projectile might be measured when the fork was vibrating most rapidly, and the velocity near the end of the flight might be measured when the fork was vibrating least rapidly, or *vice versâ*, and thus the *change* of velocity, or the measure of the resistance of the air, might be affected by a double error.

For this reason we consider that measures of the resistance

obtained by means of this instrument would be less trustworthy than those found by that of Professor Bashforth.

In order that the vibrations of the fork may be kept up by the action of the electro-magnets, it is necessary that the galvanic current which excites them should be alternately closed and broken at intervals exactly equal to the time of vibration of the fork. We understand that the adjustments which are required in order to secure this condition are somewhat difficult to make, though when they are once made the instrument works satisfactorily for a considerable time.

The cost of the instrument would be very much greater than that of Professor Bashforth's.

We are therefore decidedly of opinion that, for the purpose of determining several velocities of the same projectile and thence deducing the resistance of the air, Professor Bashforth's instrument is to be preferred to the other.

Although, as we have already mentioned, Navez-Leurs' instrument is only capable of measuring one velocity of a projectile, it is no doubt possible, by employing *two* of these instruments, to measure two velocities, one near the beginning and the other near the end of the flight of the projectile, and thus to deduce the resistance of the air.

By experiments made in this way Capt. W. H. Noble has obtained very good results, which confirm in a remarkable manner those which had been previously obtained by Professor Bashforth for high velocities, and agree approximately with those of M. Hélie for lower velocities.

It is obvious, however, that the use of two instruments instead of one is attended with a greater liability to the occurrence of constant errors. There is also a great advantage in being able to measure *several* consecutive intervals of time by the same instrument, since in this way each experiment supplies means of testing the accuracy of the results, which are wholly wanting when only two intervals of time are measured, and that by two different instruments.

With regard to the law of resistance of the air, as depending on the velocity, Professor Hélie, in his Traité de Balistique published in 1865, stated as the result of experiments with elongated

projectiles moving with velocities ranging from about 700 ft. to 1,050 ft. per second, that the resistance was nearly proportional to the cube of the velocity. Professor Bashforth, without knowing what had been done by Professor Hélie, arrived at the same law of resistance from his own experiments made in November and December 1865, and again from further experiments made in September 1866, in which the observed velocities varied in different rounds from about 1,100 ft. to about 1,500 ft. per second.

The co-efficient of resistance, however, found from Professor Bashforth's experiments is considerably greater than that obtained by Professor Hélie.

Allusion has already been made to the results obtained by means of two Navez-Leurs instruments by Capt. W. H. Noble, who, in May and June 1867, carried on for the Ordnance Select Committee a series of experiments with 12-pounder and 9-inch rifled guns, and from these experiments deduced that between the limits of 1,100 and 1,500 feet per second, the resistance appeared to vary as the cube of the velocity. The value of the co-efficient of resistance within these limits obtained by Capt. Noble confirmed with great exactness the earlier determination of Professor Bashforth. Capt. Noble also stated that there appeared to be a very rapid reduction in the resistance between 1,000 and 1,100 feet per second, and that between 600 and 1,000 feet per second the resistance again varied as the cube of the velocity; and the co-efficient of resistance obtained by him for these lower velocities agreed very closely with that found by Professor Hélie in the case of nearly similar velocities.

Between October 1867 and May 1868, Professor Bashforth made a most valuable series of experiments with elongated shot of different diameters, the charges being varied so as to give a much greater range of velocities than in his earlier experiments. The object of these experiments was to determine whether the resistance of the air might be assumed to be proportional to the cube of the velocity for all practical velocities of the shot, and also whether it varies exactly as the square of the diameter.

The results showed that while for moderate variations of velocity, such as those met with in the same round, the resistance may be taken to vary as the cube of the velocity; yet for con-

siderable variations of velocity this law no longer holds good, and that if for convenience the resistance be still represented by an expression of the form cv^3, the co-efficient c must be taken to vary continuously with the velocity. The experiments showed the values of the co-efficient c of resistance, corresponding to values of the velocity ranging from 850 to 1,600 feet per second. The co-efficient is found to attain a maximum value for a velocity of about 1,200 feet per second. Professor Hélie's value of the co-efficient of resistance is found to be true only for velocities in the neighbourhood of 950 feet per second.

Between the beginning of May 1868 and the beginning of the following November, Professor Bashforth carried out similar experiments with spherical shot, in which the velocities observed varied from 1,000 to 2,100 feet per second. The co-efficient of resistance is of course very different in the case of spherical shot from the value corresponding to the same velocity in elongated shot.

In this case also the co-efficient appears to attain a maximum value for a velocity of about 1,200 feet per second, but it falls off very considerably for the higher velocities. The resistance for the same velocity was found to vary very accurately as the square of the diameter, both for spherical and elongated shot.

We consider that these experiments of Professor Bashforth are admirably planned and that the results obtained are very valuable.

Having made these preliminary remarks, which appeared necessary in order to explain clearly our ideas respecting the merits of Professor Bashforth's chronograph and the value of the experimental results which he has obtained by means of it, we now proceed to give categorical answers to the questions which have been referred to us.

1. Whether it is now to be considered as proved that the resistance of the air varies practically as the cube of the velocity of the shot for all the velocities in use in gunnery, ranging from 300 to 1,900 feet per second, or for what range of velocities; and if not true, whether it is nearer the truth than any equally simple law before propounded?

Ans. The law can only be regarded as approximately true

for a limited range of velocities. The resistance varies most nearly as the cube of the velocity for velocities of about 1,200 feet per second; for velocities much higher or much lower than this the co-efficient of resistance varies considerably with the velocity.

For velocities within a limited range the law may be regarded as giving sufficiently approximate results, provided the co-efficient of resistance corresponding to the mean velocity be employed.

This law is attended with the important practical advantage that the calculations required by it are simple, and the results to which it leads may be readily embodied in tables.

2. Whether this law of resistance is to be regarded as a new one, the discovery of which is due to Mr Bashforth?

Ans. The law was stated by Professor Hélie in his work published in 1865, and it appears to have been in use for some years before that time in the School of Artillery of Metz, but Professor Bashforth without being aware of this, independently deduced the same law from his own experiments, so far at least as the law can be considered true, viz., for a limited range of the velocity.

Professor Bashforth by his valuable experimental determination of the co-efficient of resistance, corresponding to different velocities, has furnished the data which are absolutely necessary in order to make the law applicable in practice.

3. Whether the instrument devised and perfected by Mr Bashforth for recording successive small intervals of time is susceptible of general employment at schools of instruction in gunnery?

Ans. Mr Bashforth's instrument is simple in principle, easy to work with, and not readily liable to get out of order, and we think it well adapted for general employment at schools of instruction in gunnery.

4. Whether any means of solving the same problem with equal precision existed before?

Ans. We do not think that any means existed before of recording a number of successive small intervals of time with the degree of precision and trustworthiness attained by Professor Bashforth's instrument.

In conclusion we would beg to recommend that for the pur-

pose of rendering our experimental knowledge of the resistance of the air more complete, an additional series of experiments should be made with Mr Bashforth's chronograph, on spherical projectiles moving with smaller velocities than those which occur in his previous experiments. The results would greatly add to the theoretical interest and value of those which have been already obtained.

G. G. STOKES.
J. C. ADAMS.
A. NOBLE.

To the Under Secretary of State,
War Office.

From the foregoing Report of the Referees it is manifest that my opponents made ample use of the permission obtained by me for them, June 1869, to submit their claims, if they had any, to the Referees, to whom our experiments for velocities 900 to 1,700 f.s. had been submitted. Their ballistic officer's claims were found *to confirm* "*with great exactness the earlier* determination of Professor "Bashforth." The Director-General of Ordnance thought proper to take special care of the claims of Professor Hélie, whose experiments ranged from velocity 700 to 1,050 f.s., and appeared to indicate a resistance $\propto$ (velocity)^3. "The coefficient of resistance, "however, found from Professor Bashforth's experiments is *con-* "*siderably greater* than that obtained by Professor Hélie." So that there is a very decided difference between the results of the two sets of experiments.

The Referees had their attention directed to the chronographs of Navez, Leurs, Benton, Vignotti, and Boulengé by the Director-General of Ordnance, which were adapted to measure only a *single* interval of time.

But my opponents had still one instrument, by Schultz, which they had recommended for purchase, April 2, 1867, when they had before them evidence of the complete success of my own invention. The American price of this instrument was 900 dollars, or about £180. This was now brought forward, although they had not the *results of a single experiment* made by it, to show to the

Referees. This instrument was very properly condemned by the Referees, for reasons stated by them in their Report, 1870.

The Schultz Chronograph was used in America to measure *single* velocities of projectiles, but down to the present time I never heard of its having been employed to measure a *succession of velocities* of any one shot.

It is stated that the Schultz invention "has been much "improved, and all the objectionable features mentioned by the "Bashforth Committee have been obviated. As thus modified it "is strikingly like Professor Bashforth's Chronograph, and the "same screens, batteries, arrangement of circuits, and methods of "reduction of observations can be used in both"!

Ingall's *Ballistic Machines*, 1885, p. 29.

General Lefroy, R.A., the Director-General of Ordnance, vacated his office in the spring of 1870, a short time before the date of the award of the Referees. It has been already explained that, when my assistance was sought by the Council of Military Education, I only consented to seek office after I found that it was in my power to render efficient help, of the first importance, both to the Advanced Class of Royal Artillery Officers and to the Ordnance Select Committee. But, in April 1864, both the President and Vice-President of the Select Committee declared their hostility to my proposal for a new chronograph, adapted to determine the resistance of the air to the motion of projectiles. This opposition was continued for six years, and just up to the date of the Report of the distinguished Referees (1870), in spite of the perfect success of my invention whenever it was tried.

I regard the Report of the Referees as a most valuable document, which must have cost them a considerable amount of trouble, owing chiefly to the variety of the instruments submitted to them for their criticism, and from the distance apart of their places of residence. But they have rendered a decided service to this country, by clearing off all ignorant prejudices in favour of bad or unsuitable instruments, and in efficiently helping to lay a good foundation for the science of ballistics. This Report completely quashed the opposition to my chronograph, and I now

reprint it, with a view to preserve the good work of its authors for the benefit of all interested in the subject.

General Adye became Director of Artillery, 1870. In 1871–2 I published General Tables for facilitating the calculation of *range* and *time* of flight corresponding to a given loss of velocity of any spherical or elongated projectile

$$S_v = S_V - sd^2/w \text{ and } T_v = T_V - td^2/w.$$

"These two tables of Mr Bashforth...are universally employed "in all our text-books on gunnery."

Woolwich Text-Book, p. 168, 1902.

In 1872 there was not a sufficient number of candidates for admission to form a new Advanced Class. A Committee was thereupon appointed to consider what should be done. It was in the end decided to continue the Class, as it was felt that high scientific training was a necessity. See *Parliamentary Paper* [C. 589]. Price 8*d.*

In 1873 I published a *Mathematical Treatise on the Motion of Projectiles*, founded chiefly on the results of experiments made with the author's chronograph. Extensive tables of values X, Y, T were given with a view to render the Bernoulli solution useful in the calculation of trajectories.

I had now completed the work I proposed to do for Government, and my results had been referred to some of the first mathematicians of the day, who had given a most favourable account of them, notwithstanding the chronoscopes, &c. put forward in opposition. The Report of the Referees was duly printed, but I am not aware that any further official notice was taken of it.

I was not now disposed to make any further experiments before the value of my work was duly acknowledged officially.

This was the state of things, when I received a letter, dated March 12, 1874, from the Director of Artillery Studies, stating that he was directed to forward to me the enclosed copy of the Royal Warrant, Feb. 12, 1874, of the School of Artillery Studies, and to ask my particular attention to Articles 3 and 4 of the Warrant. From this Warrant, it appeared to me that it was

proposed to *reduce* my stipend from £550 per annum to £500. I at once sent in my resignation, a step I never regretted. Things had now come to their worst state possible.

My chronograph was sent to the Special Loan Collection of Scientific Apparatus at South Kensington Museum, 1876, by request; and, as it did not then appear probable that I should make any further experiments, I offered to present the instrument to the Museum, which was accepted.

Major-General Sir F. A. Campbell, R.A., K.C.B., late member of the Committee of the Advanced Class of R.A. Officers, 1872, and Superintendent of the Royal Gun Factories, became Director of Artillery and Stores in 1874. A short time afterwards application was made to me for the loan of my chronograph, in order to carry on our old experiments of 1868, which covered velocities of 900 to 1,700 f.s. for elongated shot. The chronograph was borrowed from the South Kensington Museum. The experiments were carried out at Shoeburyness, and the records were sent down to me in the country for reduction. The experiments made there, Sep. 1878 to March 1879, extended my coefficients of resistance to all velocities between 430 and 2,250 f.s. Tables were calculated, and the Report, dated July 8, 1879, was printed, 84/B/2853.

But, before this Report was circulated, it was found possible to carry out additional experiments made for both higher and lower velocities. These were carried out at Shoeburyness between March and July 1880, and they extended the coefficients of resistance to elongated projectiles, for all velocities between 100 and 2,800 f.s. The experiments made in 1878, 1879, and 1880 were combined, and my Final Report, dated August 1880, was printed, 84/B/2909. These experiments were carried out at Shoeburyness by

Captain C. Jones, R.A.,
Captain Bainbridge, R.A.,
Captain Morley, R.A.,
Captain McClintock, R.A.,
Captain O'Callaghan, R.A., and
Captain White, R.A.

My best thanks are due to General F. A. Campbell for thus shewing the practical utility of my invention.

I published a Supplement to my *Mathematical Treatise on the Motion of Projectiles*, 1881, in which the coefficients of resistance and tables were extended from velocity 100 to 2,800 f.s. Numerous Bernoulli Tables were also added.

In 1883 I published my *Attempt to Test the Theories of Capillary Attraction*, a work which had been suspended by my ballistic labours since 1864.

In recognition of the services rendered by me, a reward of £2000 was granted, 1885, when I was informed that "it affords "His Lordship (the Marquis of Hartington) pleasure to state, on "behalf of Her Majesty's Government, that you are considered to "have established a fair claim to substantial acknowledgment for "these services, which have undoubtedly and in a considerable "degree advanced the science of gunnery."

By the differencing of the records of all the adopted rounds I was quite satisfied with their general accuracy. But as the subsequent treatment of those readings had been carried out, perhaps rather hurriedly, I was not quite satisfied of their ultimate correctness. I therefore decided, at my leisure, to recalculate all rounds to *five* places of decimals of a second, and then deduce from these results the coefficients of resistance, to all ogival-headed projectiles ($1\frac{1}{2}$ d.) for velocities 100 to 2,800 f.s., and to spherical balls for velocities 720 to 2,280 f.s. These results were published as *A Revised Account of the Experiments made with the Bashforth Chronograph to find the Resistance of the Air to Projectiles, with the Application of the Results to the Calculation of Trajectories*, 1890. There were also given extended Bernoulli Tables of (x), (y), (t) and (v) for Newtonian and of (X), (Y), (T) and (V) for cubic laws of resistance.

In my two Supplements, published in 1895 and 1900, there appeared numerous satisfactory tests of my coefficients of resistance derived from the calculation of times of flight for the low elevations given in Range Tables. The proceedings of Mayevski and Krupp were there fully discussed, and nothing more need be said on that subject.

In the trial of my chronograph in 1865, the first two rounds were perfectly good, but, as usual, the muzzle velocity of the first round fired was much below the average. I will take the means of the times of passing each screen in rounds 1 and 2 as given in the description of the chronograph in the *Proceedings of the R.A. Institution*, 1866, for comparison with the times calculated by the tables of the *R.A. Class Book*, 1902. These calculations have been given in full in my *Historical Sketch*, 1903, p. 7.

Screen	1	2	3	4	5
	″				
Mean of rounds 1 and 2	0·0	·10545	·21195	·31945	·42800
Calc. 1902	0·0	·10522	·21121	·31874	·42755
Differences	·0	+ ·00023	+ ·00074	+ ·00071	+ ·00045
Screen	6	7	8	9	10
Mean of rounds 1 and 2	·53780	·64890	·76100	·87420	·98840
Calc. 1902	·53727	·64827	·76043	·87361	·98769
Differences	+ ·00053	+ ·00063	+ ·00057	+ ·00059	+ ·00071

Also the following is a statement of the difference between the means of the observed *times* of passing each screen in all the eleven good rounds in the trial experiments, 1865, with the times calculated by tables of 1902.

Screen	1	2	3	4	5
	″				
Tests 1865	0·0	·10489	·21106	·31845	·42705
Calc. 1902	0·0	·10522	·21121	·31874	·42755
Differences	·0	− ·00033	− ·00015	− ·00029	− ·00050
Screen	6	7	8	9	10
Tests 1865	·53684	·64780	·75988	·87309	·98831
Calc. 1902	·53727	·64827	·76043	·87361	·98769
Differences	− ·00043	− ·00047	− ·00055	− ·00052	+ ·00062

The shot used in 1865 were the *service* shot, which encountered a resistance slightly lower than those used in forming the table, 1902.

So that, *from the first*, the chronograph acted in a most satisfactory manner; in fact, for all practical purposes it was *perfect*.

It remains now only to test my Bernoulli Tables when used to calculate trajectories. For this purpose I have used a good Range Table, sent by my opponents to shew some error of mine. I use it to shew that I am right. "Range Table for 4-inch B.L. Gun. "Marks. II and III. Based on Practice of 17.5.83; 7.3.84; 21 and "23.4.84. Ogival Heads, two diameters. Projectile, 25 lbs.; "Muzzle Velocity, 1,900 f.s. Jump, 6 minutes." From the above-named table, the ranges, elevations, and times of flight have been copied below for all ranges beyond 4,000 yards as follows, so as to enable any one to test my calculations:

Range	Elevation	Time of Flight	Range	Elevation	Time of Flight	Range	Elevation	Time of Flight
yards		seconds	yards		seconds	yards		seconds
.....	.....	.....	5300	10° 11′	15·5	6600	14° 52′	21·3
4100	6° 36′	10·86	5400	10 31	15·9	6700	15 16	21·8
4200	6 52	11·23	5500	10 51	16·3	6800	15 41	22·4
4300	7 8	11·60	5600	11 12	16·7	6900	16 6	22·9
4400	7 24	11·98	5700	11 33	17·1	7000	16 32	23·5
4500	7 41	12·36	5800	11 54	17·5	7100	16 59	24·0
4600	7 58	12·74	5900	12 15	18·0	7200	17 27	24·6
4700	8 16	13·13	6000	12 36	18·4	7300	17 55	25·1
4800	8 34	13·52	6100	12 57	18·9	7400	18 23	25·6
4900	8 53	13·91	6200	13 19	19·3	7500	18 52	26·2
5000	9 12	14·3	6300	13 42	19·8	7600	19 22	
5100	9 31	14·7	6400	14 5	20·3	7700	19 52	
5200	9 51	15·1	6500	14 28	20·8			

The ranges and times of flight for elevations 7°, 8°, ..., 19°, 20° have been calculated by the help of my Bernoulli Tables. The calculations for an elevation of 7° were printed in full in my second Supplement (344), but only the leading steps for elevations 8°, 9°, ..., 20° were given. In the following table the calculated range 4,149 yards has been given in column (1), the elevation 7° used in calculation in column (3), and the calculated time of flight 10″·95 in column (6). Then, from the above Range Table, it is found that, corresponding to a range of 4,149 yards, the elevation is 6° 44′, given in column (2), and time of flight 11″·04, given in column (5). And so on for all the calculated ranges, 8°, 9°, ..., 20°.

Range	Elevation			Time		
1 R.T. and Calc.	2 R.T.	3 Calc.	4 Diff.	5 R.T.	6 Calc.	7 Diff.
yards			′	″	″	″
4149	6° 44′	7°	16	11·04	10·95	−0·09
4466	7 35	8	25	12·23	12·21	−0·02
4807	8 35	9	25	13·55	13·45	−0·10
5108	9 33	10	27	14·73	14·65	−0·08
5389	10 29	11	31	15·86	15·84	−0·02
5666	11 26	12	34	16·96	17·01	+0·05
5922	12 20	13	40	18·09	18·15	+0·06
6167	13 11	14	49	19·17	19·28	+0·11
6397	14 4	15	56	20·29	20·40	+0·11
6624	14 58	16	62	21·42	21·51	+0·09
6821	15 46	17	74	22·50	22·58	+0·08
7003	16 33	18	87	23·52	23·61	+0·09
7222	17 33	19	87	24·71	24·74	+0·03
7482	18 47	20	73	26·09	25·94	−0·15

The differences between the R.T. and calculated times of flight given in column (7) are so small that they may be neglected, and consequently the coefficients of resistance and my Bernoulli Tables appear to be quite correct. The differences of the elevations given in column (4) must be ascribed to jump and kite-like action. The 4-inch gun, laid at the following elevations, appear to have been increased by jump and kite-like action as follows:

Elevation of gun	7°	8°	9°	10°	11°	12°	13°	14°	15°	16°	17°	18°
Jump and kite-like action	16′	20′	25′	30′	35′	41′	47′	54′	61′	69′	77′	85′

If my advice had been followed from the first, the chronograph, &c. would have been constructed by a good mechanic in about six months; the trial of the instrument would have occupied a month; the experiments with elongated projectiles, with various forms of heads, a month; and the experiments with spherical and elongated projectiles, twelve months. So that, allowing four months for preparation of projectiles, &c., the whole work would have occupied just two years, as estimated.

THANKS FOR SUPPORT AND ASSISTANCE.

I WISH to express my great obligation to Government for the publication of all the reports of my experiments, with the date of firing every round annexed. It is plain that ample facilities were afforded me for *trying* and *experimenting* with my new chronograph. Having succeeded beyond my expectations, I was prepared to leave the results to be applied to practical questions by others better acquainted with the wants of the Service.

My thanks are due to Major-General Sir F. Abbott, C.B., member of the Council of Military Education. Also to the members of the Committee, which was appointed to assist in carrying out the experiments sanctioned by the Secretary of State for War, April 1867, to determine the resistance of the air to the motion of projectiles—namely, to Colonel C. F. Young, R.A., Director of Artillery Studies; to Colonel Charles H. Owen, R.A., Professor of Artillery, Royal Military Academy; to Captain J. P. Morgan, R.A., *p.a.c.*; to Captain A. Ford, R.A., *p.a.c.*; and to Lieutenant J. Sladen, R.A., *p.a.c.* At Shoeburyness, on all occasions, every possible assistance was given, for which my thanks are due to Major-General F. Eardley Wilmot, R.A.; to Major Alderson, R.A.; and to Captain Ellis, R.A. I feel much obliged for support and assistance to the officers of the Royal Artillery Institution, who did every thing in their power to help me.

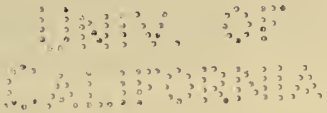
When Major-General Sir F. A. Campbell, R.A., K.C.B., was Director of Artillery and Stores, my chronograph was borrowed and used in experiments at Shoeburyness, 1878—1880.

By my experiments, 1868, the resistances of the air to the motion of elongated projectiles were found for all velocities

between 900 and 1,700 f.s., but these Government experiments extended these limits to all velocities between 100 and 2,800 f.s. This I considered to be the first official recognition of my ballistic labours, for which my best thanks are due to all concerned in the work.

My Ballistic Tables have found a place in several Woolwich Class-books, as in the *Text-book on Gunnery*, 1879, 1883, 1887, 1897, and 1902; in the *Handbook for Field Service*, 1879; in the *Manual of Gunnery of H.M. Fleet*, 1880; in the *Treatise on Military Small Arms*, 1888; and in Noble's useful *Rules and Tables*, 1882. I am deeply indebted to the editors. I feel very much obliged to Professor Greenhill, F.R.S., for his numerous good services to the cause I advocate. Further ballistic experiments were resumed, 1902–5, and the reduction of them is now in progress. Of course new questions will arise from time to time which call for new experiments.

December, 1906.



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By the same Author.

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